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ABSTRACT

Although there seems to be widespread agreement that new approaches to studying intelligence should combine the differential and cognitive (information processing) approaches in isolating components of intelligence, researchers disagree about how the approaches should be combined and, consequently, about how elementary components of intelligence should be isolated and what they are. This report proposes guidelines to help investigators make informed choices. The first section proposes guidelines for choosing among various methodologies for studying intelligence and then describes briefly some of the methods that meet these guidelines. The second section proposes guidelines for the specifications of subtheories of intelligence and illustrates how these guidelines can be met. The final section describes the direction in which the author believes these subtheories and methods should lead. (Author/AA)

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and consequently, in how elementary components of intelligence should be isolated and in what they are. How does an investigator choose from among the multiple paths available for theory and research? In this article, I propose some guidelines that may help investigators make informed choices. The article is divided into three major parts. In the first, I propose guidelines for choosing from among various methodologies for studying intelligence, and then describe briefly at least some of the methods that meet (or come close to meeting) these guidelines. In the second part, I propose guidelines for the specifications of subtheories (and eventually, full-fledged theories) of intelligence, and illustrate how these guidelines can be met. Finally, I describe the direction in which I believe our subtheories and methods should lead us.

Intelligence Research at the Interface between Differential and Cognitive Psychology: Prospects and Proposals

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Just five years ago, during the spring term of my first year in graduate school, the famous instructor of my course on Human Abilities painted a dim picture of the then current state of intelligence research. Lee Cronbach seemed to view it as his duty to inform us that research on intelligence was not in fashion. My own impression was that Cronbach was being charitable: Research on intelligence seemed to have gone the way of Nehru suits and long dresses.

My history of involvement with the field of intelligence had been less than promising: By any reasonable standard, it seemed to be one of avoidance learning. My first formal exposure to the field was in seventh grade, in a science project on the "development of the mental test." I became excited about what was to me a new and challenging discipline--the study of intelligence and its testing. My science teacher, the late William H. Adams, was most encouraging; but my ideas about experimentation, including the administration of the Stanford-Binet to my classmates, won less than hearty approval from the head psychologist of the school system, who, having gotten wind of what I was doing, promised to burn the Terman-Merrill (1937) book if I ever brought it into school again. During secondary school, I pursued my interest independently, hoping that things would pick up upon my entrance to college. They didn't. When I entered Yale is 1968, there was only one psychologist with a commitment to the study of intelligence and individual differences, and he was about to retire. And later, during my first year of graduate school, I was faced with

what seemed like an inescapable conclusion--that the whole field had retired.

But Lee Cronbach mentioned near the end of the course on Human Abilities that new ideas were just beginning to emerge regarding intelligence and how to study it, and that Lauren Resnick was organizing a conference to bring some of these ideas (and their sources) together. At about this time, I was beginning to form some ideas of my own (see Sternberg, 1977b), ideas very much in the spirit of those to be published in the proceedings of the Resnick conference (see Resnick, 1976, and especially the contribution of Carroll, 1976). The whole field had not retired; it had merely resigned temporarily until a better job could be done. And although Nehru suits never did make it back (at least not yet), long dresses, like research on intelligence, are back in fashion again.

In 1978, a diversity of new subtheories of and methods for studying intelligence confront the prospective investigator. There seems to be widespread concurrence among theoreticians and methodologists alike that new approaches to studying intelligence should somehow combine the differential and cognitive (information-processing) approaches that have been used in the past, and that the combination should somehow enable the investigator to isolate components of intelligence that are elementary (at some level of analysis). Researchers disagree, however, as to how the differential and cognitive approaches should be combined, and consequently, in how elementary components of intelligence should be isolated and in what they are. How does an investigator choose from among the multiple paths available for theory and research? In this article, I will propose some guidelines that may help investigators make informed choices. The article will be divided into three major parts. In the first, I will propose guidelines for choosing from among various methodologies for studying intelligence, and then describe briefly at least some of the methods that meet

(or come close to meeting) these guidelines. In the second part, I will propose guidelines for the specification of subtheories (and eventually, full-fledged theories) of intelligence, and illustrate how these guidelines can be met. Finally, I will describe the direction in which I believe our subtheories and methods should lead us.

METHODOLOGY

Guidelines for Methods for Studying Intelligence

In proposing methodological guidelines, I realize I risk repeating what many readers may have learned in courses on statistics and research methods. If that be the case, the repetition seems justified, because a review of much of the recent literature on intelligence reveals that in a great majority of cases, many of these guidelines are not being followed. I'm therefore prepared to run the risk of being repetitious.

Internal Validation

It was stated earlier that methodologists seem to agree that a major goal of contemporary intelligence research should be to isolate components of intelligence that are elementary at some level of analysis. Internal validation consists of the attempt to demonstrate that the components isolated in a particular analysis are consistent with the latency or error data on the basis of which the existence of the components is claimed to be supported.

Theory testing. Almost everyone now studying intelligence agrees that one's method for studying intelligence should be theory-based. But theories seem to be serving at least two very different functions in contemporary research on intelligence. On the one hand, a theory can be proposed and assumed to be correct. In this case, the theory may provide a guide for the research, but the theory is not actually tested, or subjected to potential falsification. The theory's connection to the research may be barely visible at all. On the

other hand, a theory can be proposed and hypothesized to be correct. In this case, the theory both provides a guide for the research and is tested, thereby subjecting it to potential falsification. I propose that the latter approach to theory utilization is the preferred one. Our collective experience of nearly a century of differential research on intelligence is the best guide as to why. The major obstacle to progress in differential research on intelligence may well have been the nonfalsifiability of the theories that were proposed. New theories were born, but old theories never died, and often did not even fade away. Theorists had no way of controlling the resulting population explosion, and the result seems to have been mass suffocation, if not of the theories, then of interest in them. The root of the problem seems to have been the inadequacy of the factor-analytic method to which many differential theorists were wedded as a means of falsifying theories. (Reasons for its inadequacy in this respect are discussed in Sternberg, 1977b, Chapter 2.) If intelligence research is to thrive, we need methodologies that enable us to recognize and replace outworn theories. Several criteria for recognizing such theories are described below.

Comparison of proposed theory to true theory. One criterion for rejecting a theory is inadequacy of the theory relative to the true theory. Obviously, we don't know what the true theory is, or else we would rush to propose it. But we usually do know what the true theory predicts, for example, a perfect correlation between a dependent variable and one or more independent variables (after correction of all variables for attenuation). In order to assess the proposed theory, one must know (a) the proportion of variance in the data accounted for, (b) whether the variance accounted for is statistically significant, and (c) whether the variance unaccounted for is statistically significant. (Ways of testing these are described in Sternberg, Note 1.) All three of these criteria can be applied in analysis-of-variance designs as well as in regression

designs, using ω^2 instead of r^2 or R^2 (see Hays, 1973). It is also desirable to have some absolute measure of deviation from fit, such as root-mean-square deviation (RMSD) of observed from predicted values. I am assuming here, as is almost always the case, that one's goal is to maximize the strength of the relation between two or more variables. If, for one reason or another, one's goal is to attain a moderately strong relation between two variables, then one is obliged to indicate the range of values that would falsify the theory being tested.

In comparing one's theory to the true theory, it is important to keep in mind the oft-learned but oft-forgotten fact that one can never prove that one's own theory is identical to the true theory, but merely that for a given set of data, the predictions of the two theories cannot be distinguished. There may be some third theory that is also indistinguishable in its predictions from the true theory.

Comparison of proposed theory to plausible alternative theories. In practice, the success of a theory relative to the true theory is often independent of the success of the theory in competing against alternative plausible performance theories. This point was made rather dramatically in the published sequence of research reports on the sentence-picture comparison task. Clark and Chase (1972) found that their theory of the task accounted for extremely high proportions of variance in their latency data (often over 99%), and that the theory generally could not be rejected relative to the true theory. But Carpenter and Just (1975) later showed that quite a different theory could account for the data equally well, and also fail to be rejected relative to the true theory. It is thus important to compare one's theory not only to the true theory, but to a set of plausible alternative performance theories as well. A proposed theory accounting for a given proportion of variance in the data might

be viewed favorably or unfavorably depending upon how well alternative theories do in accounting for the data. Indeed, even if a proposed theory is rejected relative to the true theory, it may nevertheless be accepted as the best working approximation to the true theory if there is no better plausible alternative.

Generality of proposed theory. In order for a theory to be of any interest, it must have some generality. It is insufficient, therefore, to propose or merely test a theory for a given task with a given content in a given experimental paradigm. Interest in the theory will be enhanced to the degree it is shown to be general across paradigms, contents, and tasks, and so the investigator should feel obliged to demonstrate generality.

External Validation

How do we know that the components of information processing that we have isolated from a particular task are components of "intelligence," or have anything to do with intelligence at all? This question must be taken seriously, since it is possible to fulfill all requirements for internal validation of a theory, and yet have a theory that has little or nothing to do with intelligence. We are beginning to see an amassing of evidence that suggests that a relatively small collection of information-processing components can account for a variety of cognitive task performances, such as visual scanning, memory scanning, stimulus matching, and the like (Chiang & Atkinson, 1976; Rose, Note 2). But does level of performance on these components relate to level of performance on the Binet-type (or other) tasks generally believed to require "intelligence"? Available evidence suggests low to moderate levels of relation at best (see, for example, Hunt, Frost, & Lunneborg, 1973, and Hunt, Lunneborg, & Lewis, 1975). These relations are investigated by external validation, through which one shows generalization or lack of generalization of patterns of individual differences from tasks that are hypothesized to measure intelligence to tasks that

are widely believed to measure intelligence.

Convergent validation. First, one must demonstrate convergent validity--that the components of interest do correlate with components or tasks that are generally accepted as measures of intelligence. Internal validation may well have shown that the isolated components are likely to be true components of the task or tasks under consideration. But if convergent validity is not or cannot be demonstrated, the scientific public has no reason to believe that the components that were isolated have any external validity as measures of intelligence.

Discriminant validation. Second, one must demonstrate discriminant validity--that the components of interest do not correlate with components or tasks that are not of interest but are possible sources of confounding. Convergent validation may well have shown that the isolated components are correlated with intelligence or some aspect of it. But if discriminant validity is not or cannot be demonstrated, the scientific public has no reason to believe that the correlation of the components that have been isolated with measures of intelligence is not due to confounding with some other uninteresting construct.

Conclusions

Some basic guidelines for research on intelligence have been proposed. These guidelines seem to be minimal ones, at least in the sense that they are the kinds of guidelines experimentalists learn about early in their careers, often in research methods or statistics courses. Yet, a review of recent published research on intelligence reveals that only a small fraction of this research follows even most of these guidelines. The most serious consequence of failure to follow the guidelines is the commission of Type II errors, which have traditionally plagued research on intelligence: We are lured into believing that a phenomenon or relationship exists when it doesn't. Other researchers are then enticed by these false leads, resulting in increasing amounts of wasted time.

We are already beginning to see in the literature reports of failures to replicate relatively recent findings that generated some publicity, and reports that phenomena that do indeed replicate are not what they seemed to be. We can be thankful, at least, that the replications are being done. But we could reduce initial false starts by following some simple guidelines for our research.

Promising Methodologies

Two to three years ago, in writing a monograph discussing available approaches to research on intelligence (Sternberg, 1977b), I was chagrined by the lack of viable options for researchers to follow. Established differential and information-processing methodologies seemed inadequate, and although I believed the methodology I was proposing (componential analysis) was adequate, a single methodology would leave researchers with little choice. Since then, however, there have been other methodological developments that I view as promising, and that seem to offer the researcher a wider choice to suit his or her research style and predilections. I will briefly describe four promising methodologies here, each of which is an outgrowth of what I believe was an unsatisfactory methodology. The four methodologies I have selected are almost certainly not an exhaustive list of promising sets of techniques. I have chosen them because (a) the sets of techniques have been well specified and shown to work in at least one instance, (b) they have the potential for meeting the guidelines described above, and (c) they meet certain criticisms of their predecessors that I outlined in my earlier work (Sternberg, 1977b) and repeat here.

An Outgrowth of the Differential Approach to Intelligence: Confirmatory Maximum Likelihood Factor Analysis and Analysis of Covariance Structures

I have previously criticized factor-analytic methodology on four grounds (Sternberg, 1977b):

1. Lack of control over mathematical realization of psychological theory. The mathematical theory is defined by the machinery of factor analysis rather than by the investigator. An investigator can go into such an analysis without any theory at all, and come out of the analysis with something resembling a theory. (The reasons for the lack of resemblance are described in the earlier work.)

2. Solution indeterminacy: the rotation dilemma. Since the axes obtained in a factor analysis are subject to arbitrary rotation, an infinite number of possible sets of rotated factors may be obtained, each of which has different psychological implications.

3. Failure to discover or explicate process. Factor analysis has generally been used to explicate the inter-item structure of multiple tests, rather than the intra-item structure of single tests. But processes occur in the solution of single items, and these single items must therefore be studied in order to elucidate the processes.

4. Interindividual nature of analysis. The components of intelligence are intraindividual--they exist within subjects, whereas factor analysis is generally interindividual--it analyzes patterns of individual differences across subjects. Since individual differences are meaningless in the context of single individuals, it is not clear how factor analysis enables us to discover components of processing inside single individuals.

At the time, I noted that confirmatory maximum-likelihood factor analysis (Jöreskog, 1969a;Jöreskog & Lawley, 1968) seemed to provide a possible way out of some of these dilemmas. Confirmatory maximum-likelihood factor analysis extracts factors with predefined characteristics, and then provides the means to determine whether the variance unaccounted for by the factors is statistically significant. The predefined characteristics of the model can be entries in the factor pattern matrix (giving loadings of variables on factors), the factor

intercorrelation matrix (giving intercorrelations between all pairs of factors), or in the matrix of unique variances (giving variances of unique, i.e., non-common, factors). Parameters of the model can be prespecified either by assigning them specific values (usually 0 or 1), or by setting them equal to each other, in which case they are free to vary within the constraints of equality.

Frederiksen (Note 3) has used a variant of confirmatory maximum-likelihood factor analysis, analysis of covariance structures (Jöreskog, 1970), to isolate component skills in reading, and appears to have done so with some success. He proposed a model with five of what he called "component factors": grapheme encoding, encoding of multiletter units, phonemic translation, automaticity of articulation, and depth of processing in word recognition. He hypothesized that if his model is correct, certain basic processes as measured by contrasts for standard laboratory cognitive tasks should load on some factors but not on others. He tested the model using covariance data from 11 response-time measures, chosen to represent the various stages of processing. He was unable to reject the proposed model ($p = .2$), although he was able to reject three alternative models ($p < .05$). The proposed model accounted for nearly all of the variance in subjects' general reading ability, as measured by standardized tests of reading comprehension.

The methodology used by Frederiksen permits adherence to all of the guidelines specified earlier, and indeed, Frederiksen followed all but one. He (a) tested a prior theory by (b) comparing it to the true theory (against which it could not be rejected) and (c) comparing it to alternative theories of the task in question (which did not fare as well). He also (d) demonstrated some generality by having multiple tasks to measure each factor. Convergent validation was accomplished by relating components of the model to scores on standardized

reading tests, although discriminant validation was ignored.

The methodology also seems to obviate at least two of the four limitations of factor analysis described above--control over the mathematical theory and solution indeterminacy. In confirmatory maximum-likelihood methods, the mathematical theory is specified in advance, so that the first limitation is overcome. The solution is unique if a total of at least f^2 elements (where f equals the number of factors) in the pattern and factor intercorrelation matrices are specified in advance. (Frederiksen did specify enough parameters in advance to insure uniqueness.) Thus, the second limitation is also overcome.

The third limitation, inability of factorial methods to discover or explicate process, can be addressed (as it is by Frederiksen's clever combination of differential and information-processing methodologies), but I have not seen evidence that it can be overcome. Rather than merely using composite tasks as variables in his factor analysis, Frederiksen used carefully constructed contrasts for the tasks, contrasts that were hypothesized to reflect elementary information processes. Frederiksen was thus able, at least to some extent, to "get inside" the tasks. However, factor analysis remains a structural model, and the factors of Frederiksen's model seem less like elementary component processes than they seem like global stages that contain multiple component processes. In this sense, they are best viewed as reference abilities--"constellations of components (latent traits) that in combination form stable patterns of individual differences across tasks" (Sternberg, 1977b, p. 78). In my own componential framework, factors are mathematical representations of reference abilities, and I believe this framework applies to maximum-likelihood as well as standard factors. The factors simply cannot be equated with elementary component processes, which in Frederiksen's research measured by subtractive contrasts. Maximum-likelihood factor analysis, then, can serve

as a useful way of testing models of the structure and interrelations of reference abilities. It does not test whether the isolated components are actually the components of reading. This can be done only by isolating hypothetical components of reading and testing whether they can account for differential latencies or difficulties resulting from manipulation of independent variables in a reading task.

The fourth limitation of factor analysis, the interindividual nature of the analysis, is also addressed but not overcome by the confirmatory maximum-likelihood methods. Frederiksen addresses this problem by analyzing aspects of intraitem structure, rather than merely using composite tasks as the variables in the analysis. But the analysis is still based upon individual differences among subjects. A possible solution to the problem might be found in the use of confirmatory maximum-likelihood methods in the context of a three-mode factor analysis (Jöreskog, 1969b), where both individuals and variables could be factored simultaneously. But the success of such a method remains to be demonstrated, so that at least for the time being, the limitation seems to stand.

Used in conjunction with information-processing techniques, confirmatory maximum-likelihood factor analysis (including analysis of covariance structures) seems to offer a promising approach toward understanding intelligence. To date, the methodology has been little tried in research on intelligence, but I believe that those with a psychometric bent will find this methodology a worthwhile one to explore.

Outgrowths of the Information-Processing or Cognitive Approach to Intelligence

During the 1970's, a large number of investigators have taken an information-processing or cognitive approach toward understanding intelligence. The goal

in this research is to discover the representations, processes, and strategies subjects use in solving problems widely acknowledged to require "intelligence" for their solution. The information-processing approach has spawned three subapproaches, which may be conveniently organized under the headings of the subtraction method, the additive-factor method, and computer simulation.

An outgrowth of the subtraction method: Componential analysis. In the subtraction method, one measures the duration of a mental event by comparing the amount of time a subject takes to solve a task requiring that mental event to the amount of time a subject takes to solve a task that differs from the first task only in the deletion of that event. I have previously criticized subtraction methodology on five grounds (Sternberg, 1977b):

1. Parameters are often confounded. Single parameters estimate latencies for multiple component processes, each of which is of interest in its own right and should therefore be disentangled from the other processes.

2. Alternative models are often indistinguishable. Alternative models are rendered indistinguishable when the component processes that could distinguish them are estimated as confounded parameters.

3. Parameter estimates are based upon too few degrees of freedom for residual. Large numbers of parameters are estimated on the basis of small numbers of data points, increasing greatly the probability of capitalization upon chance in model fitting.

4. Ordering of parameters is not mathematically specified. Although the information-processing model to be tested specifies the ordering of component processes, the model is tested in a way that places too few or no constraints on the order in which the processes actually occur.

5. Results of external validaton may be distorted. Correlational patterns of parameters with external measures can be distorted if component processes are

estimated as confounded parameters, but the underlying component processes are not themselves highly correlated across subjects. In this case, the estimated parameter is not a unitary source of individual differences.

Two more common criticisms of the subtraction method are that its use requires a sophisticated prior theory, and that its use requires the assumption of pure insertion--that in deleting a component process from a task, one is deleting this process and only this process, and that one is not changing the task in any other way. I discuss elsewhere why I believe these criticisms to be less than compelling in modern applications of the subtraction method (Sternberg, 1977b, pp. 57-60).

I have proposed a methodology for studying intelligence, componential analysis (Sternberg, 1977b, 1978), that I believe obviates or at least mitigates the force of the five criticisms presented above. The methodology has been applied to the solution of analogies (Sternberg, 1977a, 1977b; Sternberg & Rifkin, Note 4; Sternberg & Nigro, Note 5), transitive inference problems (Sternberg, Note 1, Note 6), and categorical and conditional syllogisms (Guyote & Sternberg, Note 7; Sternberg & Turner, Note 8) with encouraging results. It is currently being applied to many other kinds of problems as well.

Componential analysis provides a set of procedures to assist in the identification and testing of the (a) component processes, (b) combination rule for different component processes, (c) combination rule for multiple executions of the same component process, (d) latencies and difficulties of component processes, and (e) relations among the component processes and between these processes and higher-order reference abilities. In some applications, (f) representation of information is assumed; in others, it is tested. A central feature of componential analysis is the breakdown of a composite task into a series of subtasks, each of which requires successively less information processing. This breakdown

of the task can be accomplished in a number of different ways (Sternberg, in press). I will briefly describe here two ways in which one task, the linear syllogism, was decomposed.

A linear syllogism (also called a three-term series problem) is a kind of transitive inference problem in which a subject is presented with two premises, each describing a relation between two items. One of the items overlaps between premises. The subject's task is to use this overlap to determine a relation between the two items not occurring in the same premise. An example of such a problem is "Joe is taller than Sam. Sam is taller than Ben. Who is tallest?" I have used two different procedures to decompose this task.

In one procedure, the method of precueing, trials were divided into two parts. In the first part, the subject received precueing with part of the item; in the second part, the subject received the whole item. The subject was instructed to use the precueing to do as much information processing as possible, although the subject was not actually told how to process the information. Two precueing conditions were used. In the first, the subject received a blank field in the first part of the trial, followed by the full item in the second part of the trial. In the second, the subject received the two premises in the first part of the trial, followed by the full item in the second part of the trial. A third precueing condition, consisting of only the first premise in the first part of the trial, might also have been used (although it wasn't because it seemed unnecessary for my experimental purposes).

In the second procedure, the method of partial tasks, subjects received either full three-term series problems or two-term series problems such as "Joe is taller than Sam. Who is tallest?" Solution of the two-term series problem was hypothesized to require a subset of the processes required to solve the three-term series problem, and latencies for the two types of items were

modeled jointly.

Three theories of transitive inference--a linguistic theory, a spatial theory, and a mixture theory--were pitted against each other in a series of four experiments (Sternberg, Note 1), and the theory that won the competition was also pitted against the true theory. The mean value of R^2 for the best theory, the mixture theory, was .19 points higher than the mean value for the second best theory, the linguistic theory; the mixture theory, however, could be rejected against the true theory ($p < .05$) in three of the four experiments. Parameters of the mixture theory were correlated with each other, and with scores from tests of verbal reasoning and spatial visualization in order to ascertain which processes showed linguistic patterns of individual differences and which showed spatial patterns.

Componential analysis permits adherence to the guidelines proposed earlier in this article. In the example application, a theory was proposed (the mixture theory), and tested against both the true theory (it was usually rejected) and against the major alternative theories (it was always better). Generality of the proposed theory was demonstrated by showing its superiority across adjectives, sessions, and a variety of experimental conditions. Convergent and discriminant validity were demonstrated by showing that components hypothesized to be linguistic correlated with linguistic but not spatial ability tests, and that components hypothesized to be spatial correlated with spatial but not linguistic ability tests.

Componential analysis also addresses and reduces the force of the limitations of subtraction methodology described above. We will consider each limitation in turn.

First, parameters that would be confounded in standard application of the subtraction method are separated by the use of task decomposition. Not all com-

ponent processes are separated, however. In some cases, processes that could be separated are simply of no theoretical interest in themselves, and are left confounded in order to avert a gain in the number of parameters with no corresponding theoretical gain. In other cases, component processes cannot be (or at least have not been) separated, even with the use of componential task decomposition. Task decomposition, therefore, can reduce the number of confounded parameters, but it cannot always eliminate all such parameters. Second, the increase in the number of distinguishable parameters can permit testing of alternative models that might otherwise be confounded. Third, by jointly modeling the dependent variable for the various conditions of precueing (or partial tasks), one substantially increases the number of points to be modeled, and hence the number of degrees of freedom for residual, thereby guarding against spurious good fit. Fourth, breakdown of a task requires the investigator to specify in which subtask(s) each component process occurs, thereby constraining the ordering of parameters in the model as tested as well as in the model as conceptualized. If the investigator does not know what processes occur when, the empirical fit of the model will be reduced. Fifth, the separation of parameters that would otherwise be confounded helps guard against distortion of results in external validation. A rather dramatic example of such an outcome occurred in my research on analogies (Sternberg, 1977b), where two parameters that were separated through the use of precueing were actually negatively correlated with each other.

In summary, componential analysis seems to offer the benefits of subtraction methodology at the same time that it assuages (but does not always eliminate) its weaknesses. Like Frederiksen's use of analysis of covariance structures, it combines differential and cognitive approaches to intelligence in a way that offers more insight into intelligence than does either approach taken singly.

An outgrowth of the additive-factor method: Cognitive dependency analysis. In the additive-factor method (S. Sternberg, 1969), it is assumed that (a) when an experimental manipulation affects the latency with which an information-processing task is completed, it does so by changing the durations of one or more constituent stages of processing; (b) if two different experimental factors affect two different stages, their effects on solution latency will be additive; (c) if two different experimental factors affect a stage in common, their effects on solution latency will be interactive (that is, they will mutually modify each other's effects) (Pachella, 1974, p. 52). The additive-factor method is employed by studying an information-processing task in the context of a multifactor experimental design, where the experimental factors are chosen so as to lengthen the durations of particular stages. Inferences regarding the stage composition of the task are then made by examining patterns of additivity and interaction between factors (see Sternberg, 1977b, pp. 41-44).

I have previously criticized additive-factor methodology on four grounds (Sternberg, 1977b):

1. The method does not reveal stage duration. It can reveal the amount by which a particular experimental manipulation lengthens the duration of a particular stage, but it cannot reveal the duration of the stage itself.

2. The method provides no direct indication of stage order. As in the subtraction method, one can make plausible conjectures, but these conjectures are not actually tested.

3. The method provides no direct indication of the substantive interpretation of any stage. The nature of the stage is inferred on the basis of the kind of experimental manipulation that lengthens the duration of the stage. But to the extent that a given experimental manipulation (or set of manipulations) could lengthen various types of stages, the nature of the stage remains a matter of

conjecture.

4. The fundamental assumption regarding the means of identifying separate stages is questionable. It is quite conceivable that two factors might affect a single stage additively, or that they might affect multiple stages and interact.

Some of these criticisms are addressed (although none are fully answered) under special circumstances (temporal overlap of stages) by a generalization of the additive-factor method proposed by Schweickert (Note 9), although Schweickert's method is complex and not yet clearly applicable to research on intelligence. I know of no generalization of the additive-factor method that answers any of the criticisms in a definitive way. Calfee (1976), however, has proposed a generalization of the additive-factor method that combines aspects of the differential and cognitive approaches in a way that seems greatly to increase the power of the method as a tool for investigating intelligence.

Calfee's generalization makes two major contributions. First, it generalizes the additive-factor method to the multivariate case, allowing for the possibility that multiple measures of particular processes may be employed in a multifactorial experiment. Second, it distinguishes among six different sources of dependency in cognitive processes that seem in at least some instances to be confused in contemporary research on intelligence (and other constructs). These sources of dependence were initially investigated in the context of an experiment on motor ability that unfortunately was not designed as a test of the methodology, and which did not in fact provide a particularly good test of it. Children in kindergarten and first grade were asked to draw connecting paths from drawings of rabbits to drawings of carrots. Each item consisted of a picture of a rabbit, a straight path, and a drawing of a carrot. The paths were bounded by lines that were either relatively close together or relatively far apart. Problems were administered either in a neutral set (stressing neither accuracy

nor speed) or an accuracy set (stressing accuracy over speed). The two basic independent factors of interest were path difficulty (easy, wide path versus difficult, narrow path) and instructional set (neutral versus accuracy). The two basic dependent variables were errors and latencies for drawing. An error was scored if the child either touched or went outside the boundary lines of the path. The two hypothetical stages of interest were labeled accuracy of movement and rate of movement.

Calfee hypothesized that task difficulty should affect the accuracy of movement stage but not the rate of movement stage, and that this effect should show itself in drawing errors but not in drawing latencies. He also hypothesized that instructional set should affect the rate of movement stage but not the accuracy of movement stage, and that this effect should show itself in drawing latencies but not in drawing errors. The effect of each independent variable was hypothesized, therefore, to be upon one stage but not the other, and the validity of the hypotheses could be tested by determining whether each independent variable affected only the dependent variable linked to the corresponding stage.

Calfee investigated six sources of independence in cognitive processing (see Calfee, 1976, p. 35):

1. Process independence averaged over subjects. The question addressed here is whether any between-process source of variance is so large, on the average, that the hypothesis of process independence is untenable. This is the source of independence typically investigated in additive-factor analyses. Calfee proposes testing main effects of the factors against interactions of these main effects with subjects. (A residual error term such as replications would be better, since the interaction puts individual differences in strength of effect into the error term.) In the example, process independence would be

supported by a significant effect of either (a) task difficulty on drawing errors or (b) instructional set on solution latency. Process independence would be counterindicated by a significant effect of either (a) task difficulty on solution latency or (b) instructional set on drawing errors.

2. Process independence for individual differences. The question addressed here is whether any between-process subject-factor interactions are so large that the hypothesis of process independence is untenable. Here one tests subject by treatment interactions against a residual error term. Again, significance of certain effects is consistent with process independence, but significance of other effects is inconsistent with it. In the example, process independence would be supported by a significant interaction of either (a) subjects with task difficulty for drawing errors or (b) subjects with instructional set for solution latency. Process independence would be counterindicated by a significant interaction of either (a) subjects with task difficulty for solution latency or (b) subjects with instructional set for drawing errors.

3. Intraprocess parameter independence. The question addressed here is whether parameters representing effects of within-process factors are correlated or not. It is possible to obtain for each subject and to intercorrelate parameter estimates computed as contrasts for the effect of each factor. These parameter estimates represent the additional difficulty or latency attributable to the experimental manipulation of each factor. In the example, only one factor is hypothesized to affect each stage of processing, so it is not possible to test intraprocess parameter independence. Were there at least two factors hypothesized to affect each stage of processing, however, process independence would be indicated by nonsignificant correlations between the two (or more) factors affecting each stage. Suppose, for example, that an additional Factor X were hypothesized only to affect rate of movement (and to manifest this effect through

solution latency), and an additional Factor Y were hypothesized only to affect accuracy of movement (and to manifest this effect through drawing errors). Then intraprocess parameter independence would be supported by nonsignificant correlations between the contrasts for (a) instructional set and Factor X, and (b) task difficulty and Factor Y.

4. Interprocess parameter independence. The question addressed here is whether parameters representing effects of between-process factors are correlated or not. In the example, one would test whether the contrast for instructional set (hypothesized to affect rate of movement) is significantly correlated with the contrast for task difficulty (hypothesized to affect accuracy of movement). Interprocess parameter independence would be supported by a nonsignificant correlation.

5. General parameter independence. Calfee refers to total (composite) scores as general parameters (and the contrast scores as specific parameters). The question addressed here is whether total scores for the different measures are correlated or not. In the example, one would correlate solution latency with drawing errors. General parameter independence would be supported by a nonsignificant correlation.

6. General-specific parameter independence. The question addressed here is whether general parameters (total scores) are significantly correlated with specific parameters (contrast scores). In the example, one would correlate the solution latency and drawing error measures with the contrasts for instructional set and task difficulty. Obviously, the general and specific parameters hypothesized to affect a given stage will exhibit some degree of artifactual correlation, since they are computed on the basis of the same dependent variable for the same factor. Process independence across factors, however, would be supported by nonsignificant correlations.

Calfee presents a wealth of data regarding the outcomes of the experiment. For our purposes, the main ones of interest are that process independence was rejected for sources 1, 2, 5 (at the kindergarten level), and 6. It was supported for source 4.

The demonstration experiment followed some but not all of the guidelines proposed earlier. Calfee did propose a prior theory, which he tested through an analysis of sources of cognitive dependency. Note that the form of the theory, and what is being tested, differs in this example from each of the two previous examples (which, in turn, differed from each other). Here, one is testing a hypothesis regarding the plausibility of a stage model by analysis of the various sources of dependency. Calfee also tested his theory against the true theory--that theory which would predict the patterns of dependence and independence shown in the data. His theory was rejected. Calfee did not compare alternative plausible theories of information processing, although he might easily have done so by proposing alternative patterns of dependence and independence that would form equally plausible or almost as plausible theories. Generality of the theory was not demonstrated. Although Calfee proposed and performed extensive analyses of individual differences, they were all internal. Tests of intraprocess and interprocess parameter independence might be seen as providing weak tests of convergent and discriminant validity if one takes the position that intraprocess parameters should be highly intercorrelated (showing convergent validity) and interprocess parameters should be uncorrelated (showing discriminant validity). This position has no clear justification, however. It would be a small step to incorporate external measures of appropriate abilities into the experimental design, and to test convergent and discriminant validity by correlating specific parameters with these measures.

As mentioned earlier, neither cognitive dependency analysis nor any other generalization of the additive-factor method satisfactorily deals with the limitations of the method that I have noted. With provisions made for external validation, however, cognitive dependency analysis, although barely tested, seems to be a promising methodology. It provides a unique and well-integrated combination of information-processing notions of process independence (the first two kinds listed) with differential notions of process independence (the last four kinds listed), and further distinguishes among kinds of process independence within each of these general categories. I believe the methodology is well worth further pursuit, perhaps in some kind of combination with componential analysis.

An application of computer simulation. In computer simulation, one seeks to create or test models that mimic human behavior on the computer. Ideally, patterns of data generated by "computer subjects" will mimic those of human subjects. I have previously criticized computer models on three grounds (Sternberg, 1977b):

1. Computer theories are inaccessible to the scientific public. The theories seem by their nature to be particularly inaccessible, both physically, and for most psychologists, conceptually. Verbal descriptions often fail to make clear just what the empirical claims of the theories are. The inaccessibility of the theories may thwart the public nature of the scientific enterprise.

2. Computer theories lack parsimony. Whereas parsimony for its own sake is of questionable value, computer theories are often so complex and interactive that it is difficult to separate the psychological claims made for such theories from the mechanical bookkeeping details that are needed to build a working program but that have no psychological relevance.

3. Computer theories do not generally provide process parameter estimates.

If one wants to find out the latency or difficulty of a particular component process, this information may be difficult to obtain because of the large number of interactive variables operating in most computer programs.

In my earlier discussion, I did not clearly distinguish between two uses of computer simulation that I am now convinced ought carefully to be distinguished. The first is the use of computer simulation as theory; the second is the use of computer simulation as a means of testing theory. I believe the criticisms I leveled against computer simulation as theory apply equally well today as they did when I first leveled them. But as a means of testing theory, computer simulation is as important and useful as any other means, since it provides a way of testing theories that while well specified in information-processing terms, are of a complexity that does not permit ready derivation of straightforward mathematical predictions. I will discuss in this section what I consider to be an excellent example of computer simulation used as a means to test a subtheory of intelligence, in this case, problem solving in the solution of a class of what are sometimes called MOVE problems. Although the authors did not combine differential and information-processing approaches to intelligence in their work, such a combination could be effected and increase the power of what is already a most impressive analysis.

Jeffries, Polson, Razran, and Atwood (1977) have proposed a theory that accounts for both legal and illegal moves in the solution of several variants of both water-jug problems and missionaries and cannibals problems (see also Atwood & Polson, 1976). In water-jug problems, subjects are presented with three jugs of varying capacity for holding water. Initially, the largest jug is full and the two smaller jugs are empty. The subject's task is to determine a series of moves (that is, pouring operations) that will evenly divide the water between the largest and middle-size jugs. In missionaries and cannibals problems

(also known as water-crossing problems), three missionaries and three cannibals wish to cross from one bank of a river to the other. A boat is available for this purpose, but it will hold only two travelers at a time. The subject's task is to determine a series of moves (that is, traveling combinations of missionaries and cannibals) that will transport the individuals across the river, under the constraint that the number of cannibals on either side of the river can never exceed the number of missionaries, since if cannibals outnumber missionaries, the cannibals will eat the missionaries. Both the water-jug and missionaries and cannibals problems can be varied in details while retaining the same basic problem structure.

Polson and his colleagues had subjects solve several variants of both types of problems. They proposed a detailed theory of how subjects solve the problems, which assumes that subjects integrate information from two sources--evaluation processes, by which the problem solver examines the desirability of a move and its resulting state, and memory processes, by which the subject retrieves information about previous entries into a given problem state. The actual move-selection process is divided into three stages: consideration of acceptable moves, finding of a move leading to a new state, and an attempt to select an optimal move. A move can be found in any stage, leading to self-termination in the sequence of stages. Six information-processing parameters are specified, four of whose values are hypothesized to be common across variations of each task, and two of whose values are hypothesized to be specific to each task variant. Interestingly, the common parameters proved to have remarkably similar values when estimated separately for the two types of problems, suggesting that these parameters are remarkably stable across at least two basic types of MOVE problems.

Jeffries et al. (1977) and Atwood and Polson (1976) used computer simulation to estimate parameters and test their theory. The difference between mean

numbers of predicted and observed moves was not significant in any experiment. In all comparisons, the theory appears to have given an excellent account of the data.

In their research, the authors followed most of the guidelines proposed earlier in this article. They started with a well-specified theory of problem solving. They then tested the theory against the data, and found that it could not be rejected (for group means, although it could be rejected for other statistics). They did not perform quantitative comparisons of their theory against any others, largely because theories proposed previously were not specified in the kind of detail that would have permitted such tests. They did perform qualitative comparisons with other theories, however, and these comparisons supported their own theory. The authors demonstrated the generality of the proposed theory by showing its applicability to two types of MOVE problems, and to variants of each problem type. Unfortunately, the authors did not compare parameter estimates for individuals to scores on any external measures. Such comparisons would have been most informative, however, as a means of determining which parameters of the theory capture those aspects of problem solving that contribute to general intelligence, and which capture aspects of problem solving that may be specific to MOVE or similar problems and not of much interest beyond such tasks. Comparisons of this kind could be readily carried out.

Computer simulation, as used by Polson and his colleagues, is immune to the specific criticisms of computer theories listed above, because computer simulation is used as a means of testing a theory rather than as a means of realizing a theory. Whereas a computer theory becomes inaccessible to the scientific public if it is literally embedded inside a computer, the present theory is not so embedded, and the authors provide sufficient information in their article for others to test and extend (or refute) the theory. It is clear from the presen-

tation just what psychological claims are being made. The theory is parsimonious in its use of just six parameters to model performance on a complex task. Assumptions regarding the psychological origins of these parameters are plausible and not unreasonably complex. Finally, process-parameter estimates are provided, indeed, are calculated by computer.

Polson and his colleagues are by no means the first to use computer simulation as a means of testing a theory. I found their work to be particularly elegant, however, in a substantive domain (problem solving) and methodological domain (computer simulation) each of which historically has often lent itself to work lacking in elegance and certain kinds of simplicity. With the incorporation of differential techniques of external validation, the methodology employed could provide a fruitful way of studying intelligence.

Conclusions

The question for methodologists to address today seems not to be whether differential and information-processing approaches to intelligence should be combined, but rather how they should be combined. The four methodologies described above seem like promising starts in this direction, although in some cases I have taken the liberty of suggesting ways in which further integration (or in the case of Polson and his colleagues' research, any integration) might be obtained. Differential methodology by itself is inadequate: It fails to elucidate the processes and strategies that constitute a large part of what we mean by intelligence. Information-processing methodology by itself is inadequate: It does not provide a means for systematically studying correlates of individual differences in performance, and is particularly susceptible to overvaluation of processing components that are task-specific. The virtues of each methodology are the flaws of the other, so that a skillful synthesis of the two methodologies can offer considerable promise indeed.

THEORY

Guidelines for Subtheories of Intelligence

Differential and cognitive psychologists have followed rather different strategies in theorizing about intelligence. Differential psychologists attempted to synthesize full-fledged theories of intelligence even from the earliest stages of research. Cognitive psychologists generally started with smaller subtheories of performance in particular tasks, or occasionally, classes of tasks (although there have been exceptions, such as Anderson, 1976). The direction in contemporary research integrating the differential and cognitive approaches seems to be toward subtheories of intelligence that are moderate in scope. These subtheories of intelligence take the form of theories of aspects of intelligence such as reading, reasoning, spatial visualization, and the like, or of large chunks of each of these sets of behaviors. I will briefly describe in this section some guidelines for what ought minimally to be included in such theories.

Specification of Representation of Information

As its name implies, information-processing psychology has stressed detailed specification of the processes people exhibit in various kinds of behavior. Processes must act upon some kind of representation, however, and one cannot well understand information processing without understanding the representation(s) upon which the processes act. Nowhere has this been more clear than in the debate regarding the representations and processes used in solving transitive inference problems, where claims regarding representation and process have been interdependent without the relations between them being made clear (see Sternberg, Note 1, Note 6).

Information may be represented in different ways for different tasks, or for the use of different strategies in solving the same task, or for different

stages of processing within a given strategy applied to a single task. Thus, there may be no point to debates regarding whether information is represented in a way that is discrete or continuous, imaginal or linguistic, like a network or set-theoretic. The distinctions are meaningful only in the context of particular processes applied to particular tasks solved by particular strategies.

Identification of Component Processes

Just as processes cannot be understood fully without understanding of the representation(s) upon which the processes act, so is the concept of representation incomprehensible without a specification of the processes acting upon the representation. Indeed, we have no experimental access to the form a representation might take except through the study of processes that act upon that representation. In a subtheory of intelligence, therefore, the theorist should identify a set of component processes that is sufficient to account for the domain of behavior under investigation.

Specification of Strategies for Combining Component Processes

In specifying one or more strategies by which subjects may combine component processes, the theorist must indicate both the order and mode of component execution. The specification of order must include description of all branching points and feedback loops in the chain of command. The specification of mode must indicate whether processes are serial or parallel, exhaustive or self-terminating, and independent or dependent. These specifications, in turn, must be made both for combination of different component processes and for combination of multiple executions of the same component process.

Strategy for combining different component processes. Consider three different component processes, x, y, and z, used in the solution of some type of problem. The processes may be executed in any of 3! different orders. Moreover, the processes may be executed serially (for example, x, then y, then z),

in parallel (x, y, and z occur simultaneously), or in some combination (for example, x and y occur simultaneously, followed by z). The processes may also be executed exhaustively (x, y, and z always performed) or with self-termination (for example, x and y always performed, but z performed only if x and y fail to yield a solution). Finally, execution of the processes may be independent (the occurrence of x is uncorrelated across item types with the occurrence of y, which is in turn uncorrelated across item types with the occurrence of z), dependent (x, y, and z always co-occur, so that their occurrences are perfectly correlated across item types), or partially dependent (x, y, and z tend to co-occur or not to co-occur across item types). Processes that are fully dependent will be confounded in parameter estimation procedures, since they cannot be disentangled.

Strategy for combining multiple executions of the same component process. Consider three repetitions of the same component process, x_1, x_2, and x_3, that occur during solution of some type of problem. The same distinctions that were discussed above for different component processes can be applied also to multiple executions of a single process. The same examples apply, except for the substitution of x_1, x_2, and x_3 for x, y, and z respectively.

Provision for Estimation of Component Latencies and Difficulties

A theory should be capable of specifying the latency and difficulty of each component process, although such specification need not be made in advance: Parameters corresponding to process latencies and difficulties will usually be estimated from one's data. In some kinds of methodologies, such as those deriving from the additive-factor method, it will be possible only to specify the amount by which each experimental factor lengthens the latency or increases the difficulty of each process, again by estimation of (contrast) parameters from the data.

Specification of Relations of Component Processes to Each Other across Subjects

A theory should indicate which processes will demonstrate correlations with

each other across subjects in latency and difficulty. For example, if processes x and y both operate upon a linguistic representation, and z operates upon a spatial representation, or if processes x and y are both encoding operations, and z is a feature-comparison operation, then one might expect x and y to be correlated with each other but not with z. Note that this aspect of the theory indicates dependence versus independence of processes across subjects, whereas an aspect of the theory discussed above under the heading of "strategy" indicated dependence versus independence of processes across item types.

Specification of Relations of Component Processes to External Reference Abilities

A theory should specify external reference abilities with which each component process should and should not be correlated across subjects. For example, if a process x involves selective encoding of letters presented in strings, its latency should probably be correlated with scores on standardized measures of perceptual speed, but not with (or only poorly with) scores on standardized measures of letter-series extrapolation.

Conclusions

Six guidelines, including one subdivided one (for specification of strategy or strategies), were proposed for adequate specification of a subtheory of intelligence. Obviously, this is by no means an exhaustive list of guidelines for "good" theories, but this seems like a reasonable minimal set. The reasons for following these guidelines are much the same as those for following the methodological guidelines proposed earlier, in particular, the minimization of the probability of Type II errors in research. If a theory can be shown to provide the required predictions, and to be successful in at least most of these predictions, then it seems like a good foundation upon which to build in further research. To the extent that a theory fails to make or makes incorrectly the required predictions, it seems less likely to provide a solid foundation.

A Brief Illustration of the Guidelines Applied to Research on Intelligence

I would have liked to have placed here a section entitled Promising Theories that paralleled the earlier section entitled Promising Methodologies. The conspicuous absence of such a section is perhaps a reflection of two factors. First, research combining the differential and cognitive approaches to intelligence is of relatively recent origin, and so there has not been much time for cognitive-differential theories that inspire overwhelming confidence to develop. Second, and paradoxically in some respects, the success of extant methodologies can be gauged in part by the rate of turnover in theories. Differential methodology, for example, was ultimately unsuccessful in large part because it could spawn theories but not bury them.

Under the present circumstances, I will have to be content to present briefly an illustration of the proposed guidelines for theories. The illustration, my mixture theory of transitive inference mentioned earlier in the article, is described in detail elsewhere (Sternberg, Note 1). The theory has the desirable characteristic of being superior to other existing information-processing theories in making the predictions about data required by the guidelines; it has the undesirable characteristic of being wrong, that is, of being rejected in most comparisons against the true theory.

The theory, it will be recalled, applies to transitive inference problems such as "Joe is taller than Sam. Sam is taller than Ben. Who is tallest?" According to the theory, information about relations between terms is represented both in terms of linguistic deep structures and in terms of spatial arrays. Thus, "Joe is taller than Sam" would be represented, ultimately, both as (Joe is tall+; Sam is tall) and as $\frac{\text{Joe}}{\text{Sam}}$. The theory specifies ten component processes that can be used in solving transitive inference problems, and is presented in two forms, as an information-processing flow chart and as a linear mathematical model. The

flow chart specifies the strategy subjects are theorized to use in combining both different processes and multiple executions of the same processes. The linear model permits estimation of the durations and difficulties of some (but not all) of the processes subjects are theorized to use in solving the problems. According to the theory, component processes fall into two major classes, those that operate upon the linguistic representation and those that operate upon the spatial representation. Thus, it predicts that processes acting upon the linguistic representation will be intercorrelated across subjects; that processes acting upon the spatial representation will be intercorrelated across subjects; but that processes acting upon different representations will be intercorrelated only minimally or not at all. Furthermore, it predicts that processes acting upon the linguistic representation will be correlated across subjects with tests of linguistic abilities but not of spatial abilities, and that processes acting upon the spatial representation will be correlated across subjects with tests of spatial abilities but not of linguistic abilities. In general, the various predictions of the theory are borne out. Although the theory does not account for all of the reliable variance in the latency data, it does account for almost all of it. Error rates on transitive inference problems are low, so that the same kind of detailed model testing cannot be applied to the error data as can be applied to the latency data.

THE FUTURE OF RESEARCH ON INTELLIGENCE

My vision of the future of research on intelligence is component-centered. I believe our two major tasks will be to identify the components that constitute part of what we consider "intelligent behavior," and to discover relations between these components and other constructs.

Identification of the Components of Intelligence

Like Carroll (1976) and Simon (1976), I foresee our compiling a relatively small catalogue of information-processing components that in various combinations account for performance on tasks requiring intelligence (see Sternberg, Note 10, for a description of my current partial catalogue). The catalogue will be a difficult one to compile. First, we wish it to include all those components but only those components that are psychologically interesting in the sense that they are general to a nontrivial class of tasks. There may well be a multitude of component processes that are specific to single tasks and minor variants of them. Such components will add bulk that is of little value to any general theory of intelligence. Second, we wish to include in our catalogue only those components that are nonredundant. In order to guard the uniqueness of each entry, we must take considerable care in our research to distinguish different components on the one hand from aliases of the same components on the other. Our techniques for assessing the identity of processes across tasks are still being developed, and this development will prove increasingly more important as we generalize our theory to larger and larger classes of behavior. Third, we must seek a level for these elementary processes that strikes the proper balance between molecular and molar units of behavior. The level we choose to call "elementary" is arbitrary. Components, like factors, can probably be split almost indefinitely. We need to find the point at which we start attaining diminishing returns, and avoid splitting processes beyond that point.

Relations of Individual Components to Other Constructs

Relations of Components to Other Components

We will want to know the interrelations among components both across tasks or item types within tasks and across subjects. Indeed, we will need such information as a prerequisite for understanding the relations between components and other constructs, such as factors. General and large group factors arise,

for example, when component processes that tend to co-occur across large classes of tasks are highly intercorrelated across subjects.

Relations of Components to Tasks

We will want in our catalogue a detailed index that specifies which components occur in the accomplishment of which tasks, meaning that we need a carefully constructed taxonomy of tasks. Interestingly, one of the first to appreciate this fully was Guilford (Guilford, 1967; Guilford & Hoepfner, 1971). On the one hand, Guilford's research has been highly susceptible to the pitfalls of differential methodology (see Sternberg, 1977b, pp. 31-32; Undheim & Horn, 1977). On the other hand, Guilford realized better than any other differential psychologist, and better than many information-processing psychologists, that a theory of tasks is prerequisite for a theory of intelligence. Carroll (1976) appropriately subtitled his article "A new 'structure of intellect,'" since it was certainly Guilford in his structure-of-intellect model who began the careful faceted classification and cross-classification of tasks that is continuing today.

Relations of Components to Factors

We will also want in our catalogue a second detailed index, one that specifies which components constitute which factors (see Carroll, 1976). Any new theory of intelligence will have to subsume the factorial theories of the differentialists. I believe this can be done, because the factorial theories, at least in many cases, can be viewed as different perspectives on a single theory rather than as different theories. Much of the difference in perspectives derived from disagreements regarding the preferred rotation of factorial axes (see Sternberg, 1977b, pp. 24-25, 31-32). Thus, one might repeatedly see a single factorial space appearing under many different guises. I doubt there is any one rotation that is always preferred: The preferred rotation is a matter

of convenience for a particular purpose. Through reference-ability modeling--the use of multiple regression to predict factor scores from component scores--the choice of factorial axes ceases to be a matter of much importance. The true components can account for the obtained factors, regardless of which rotation is used.

Relations of Components to Strategies

Like Estes (1976), I doubt that intelligence can be equated with a set of component processes. Part of intelligent behavior is the selection and execution of strategies that optimize task performance. It is for this reason that a theory of intelligence must take care to specify in detail the strategy or strategies subjects use in solving various classes of problems. Whereas I have found individual differences in strategies to be minor in the reasoning tasks I have studied (Sternberg, 1977a, 1977b; Sternberg & Rifkin, Note 4; Sternberg, Note 1; Guyote & Sternberg, Note 7), others have found them to be of major importance, for example, Simon in his work on problem solving (Simon, Note 11).

Relations of Components to Intelligence

Finally, we come to the most important relation of all, that of components to intelligence. Would an understanding of all of the above lead us to an understanding of intelligence? I'd like to think so, but I feel obliged to interject a note of caution. In my own research, I have rather consistently found that the best predictor of scores on reference ability tests measuring significant aspects of intelligence is the residual or constant component, the component that is estimated as the intercept of a linear regression model after all of the slopes have been accounted for (see Sternberg, 1977a, 1977b, for examples). Similar findings have been reported by others (Hunt, Lunneborg, & Lewis, 1975; Lunneborg, 1977; Egan, Note 12). On the one hand, we can feel

pleased to be rediscovering Spearman's g in information-processing terms. On the other hand, it is a sobering thought to realize that it is precisely this g that is unexplained by our models: We may not know just what it is any better than Spearman (1927) did half-a-century ago. So as we enter the second half-century following the publication of Spearman's landmark book--perhaps the first published work to adumbrate our current syntheses of differential and cognitive approaches to intelligence--we find our greatest challenge to be the same one that faced Spearman: to understand the nature of g.

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Footnote

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